MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLII. APRIL 14, 1882. No. 6.

E. J. STONE, Esq., M.A., F.R.S., President, in the Chair.

Robert T. Pett, Esq., Royal Observatory, Cape of Good Hope,

was balloted for, and duly elected a Fellow of the Society.

Remarks on the Instructions for Observing the Transit of Venus formulated by the Paris International Conference. By Professor Simon Newcomb.

The recommendations of the Conference which met at Paris in October 1881 are entitled to such weight, both on account of the eminence of the astronomers who took part, and the advantages they naturally derived from mutual discussion, that I have hesitated in suggesting any possible improvement of their carefully-prepared instructions. This hesitation has been increased by the fact that my suggestions cannot now, as I should desire, be criticised by the Conference, to which I should have been glad to submit them. I submit them now to the Royal Astronomical Society with the hope that they will receive that critical discussion by all astronomers interested which alone can lead to a decision of all the questions involved.

The strongest reason for this course is, that as the entire Transit is visible on this continent, it is not improbable that the observations by astronomers in this country will exceed in number those made by the expeditions to be sent out by foreign Governments. The question whether American observers shall

adopt the Instructions of the Conference unchanged is therefore an important one, which can be decided only by a careful examination and discussion of the subject. The following is to be regarded simply as my individual contribution to this discussion.

The Articles recommended by the Conference are found in the *Comptes Rendus* of the French Academy for October 17, 1881, tome xciii. p. 569.

The first three Articles call for little important remark, as they refer principally to the precautions to be taken by the observer; and I am unable to suggest any decided emendation. It may, however, be remarked that the use of the silvered objective might be inconvenient in the event of the Sun's rays being so obscured by smoke or haze that a deeply-coloured shade would be unnecessary. It might then be found that the silvering darkened the Sun's light too much.

In respect to Article III. I feel some doubt whether the degree of illumination of the solar disk is best defined by the distance apart at which two spider lines can be seen projected on the Sun. If, indeed, the amount of contrast between the brilliancy of the Sun and that of the atmosphere immediately around the limb were always the same—if, in fact, all the observations were made through a very transparent atmosphere, such a method of securing uniformity would perhaps be the best. But, in the actual observation of contact, everything depends upon the observer seeing the gradual approach or separation of the sharp cusps around *Venus*, and estimating the moment at which the thread of light becomes visible or invisible. Now, the greater the brilliancy, so long as the comfort of the eye is not interfered with, the more definitely, it would seem, must such phases as these be discriminated. It therefore appears to me that a better rule for regulating the brilliancy of the solar disk would be to make it as bright as the eye could bear with entire ease and comfort. To increase the precision of the observation, the Dawes solar eyepiece, cutting off all that portion of the Sun distant from the point of contact, might have been recommended.

In Article IV. we find the times of internal contact defined as follows:—

"*A l'entrée*: le moment où l'on voit pour la dernière fois une discontinuité bien évidente et en même temps persistante dans l'illumination du bord apparent du Soleil, près du point de contact avec *Vénus*.

"*A la sortie*: le moment de la première apparition d'une discontinuité bien marquée et persistante dans l'illumination du bord apparent du Soleil près du point de contact."

I confess to some difficulty in forming a precise conception of what should be understood by the phrase "illumination of the apparent limb of the Sun" and how "discontinuity" in that illumination should be noted. It may be that on actually

observing the approach of a contact, it would be sufficiently clear to the observer what was meant. At the same time I deem it proper to point out the source of the apparent difficulty. To do this, and to discuss the subsequent explanations made by the Conference, I shall have to begin with some general statements respecting the phenomena of contact, founded partly on numerous observations of an artificial Transit of *Venus* in 1874, partly on the general description of the observers of that Transit, and partly on the reason of the case.

In doing this I may refer in passing to the paper in the *Monthly Notices* for March 1877 (vol. xxxvii. p. 237).

The moment of true internal contact is that at which the sharp points of the cusps begin to meet around the dark body of *Venus* as it enters upon the solar disk. Now, if the limb of the Sun presented itself to the vision as a distinct mathematical line, a discontinuity in which could be observed with geometrical precision, there would be no difficulty in the interpretation. As a matter of fact, however, it needs little consideration to show that no such definiteness is possible. The imperfections of vision in the eye, the instrument, and the atmosphere, all combine to render it impossible. The result is that, as *Venus* is approaching internal contact, it is impossible to fix the precise point at which the supposed sharp cusps terminate. To fix such points is very much like trying to fix the point where the tail of a comet terminates. The cusps can be traced further according to the sharpness of definition, the steadiness of the atmosphere, the keenness of the eye, and the habit of the observer.

The result is that there is no one moment at which all observers, even under the same conditions, would agree that contact had occurred.

The ends of the cusps assume a certain haziness, and the distance to which each cusp can be traced depends upon the relative brilliancy of the Sun's limb and of the atmosphere around it.

Moreover, owing to the effect of atmospheric tremors, the very sharp cusps, just before internal contact, are broken up and diffused in various ways. The result is that internal contact does not show itself by the completion of what can be recognised as the limb of the Sun, but by the gradual combination of the waving and ill-defined lines of light which form the cusps. Confining the attention to the space immediately behind the planet, the discontinuity of the limb is resolved into continuity by a slow and gradual process, the region along which contact occurs being occupied by a waving mixture of light and darkness as it were. During a period after internal contact, which varies from a few seconds to a minute, according to the circumstances of vision, the minute portion of the Sun's disk visible behind the planet is broken, and darkened by the effect of irradiation, atmospheric undulation, &c. Now, during this gradual process from complete discontinuity of the Sun's limb

to complete continuity, extending over a period of perhaps an entire minute, how is the observer to say at any moment whether there is or is not "une discontinuité bien évidente"? Indeed, what should he consider a discontinuity?

This difficulty the instructions seek to resolve by describing more fully what the observer is to look for under different supposed conditions. To analyse these several instructions we must premise some further considerations respecting the character of the phenomenon.

I remark, first, that the manner in which dark appearances, such as the black drop, are very often spoken of, does not seem to be that most conducive to entire precision on the part of the observer. The only phenomena actually seen are those of greater or less degree of illumination, and darkness can only be predicated of those portions of the field of vision where there is no visible illumination. Darkness, in a word, is not visible.

As an application of these considerations I may say that the asserted presence of a black drop can mean nothing more than that the sharp cusps of *Venus*, instead of appearing sharp, as in their true form, appear rounded at their termini. May it not be, therefore, that precision would be gained if the observer's attention were confined to the phenomena of illumination exhibited by the cusps, and the black drop, or other forms of distortion, were described only as a blunting, rounding, or diffusion of the cusps?

Let us now take up the instructions in order:—

I. "Si les bords des deux astres viennent au contact géométrique sans déformation et sans obscurcissement du filet de lumière interposé, l'instant défini est celui même de ce contact."

This statement seems to me formulated with entire precision, and therefore does not call for further remark.

II. "S'il se produit une goutte noire ou ligament, bien net et aussi obscur que le corps même de la planète, les instants définis précédemment sont, à l'entrée celui de la rupture définitive, à la sortie celui de la première apparition du ligament."

I cannot but fear that in this case observers will note the time of ingress so much too late, and that of egress so much too early, as to destroy the value of their observations. All experience shows that there is no precise moment of rupture or formation of the ligament in such a case. Observers do, indeed, very often describe the formation as sudden, but, as is well known to all who have investigated the subject, the times they give for that sudden formation are so discordant that no weight can be assigned to it. In fact, as pointed out in the paper already referred to, if we suppose the planet approaching interior contact at egress, the band of light in front of it, as it grows thinner, appears darker to the vision from various optical causes too obvious to need enumeration. The amount of darkness is, however, uncertain, or, to speak with more precision, the brilliancy of the band as it fades away diminishes very gradually,

until near the time of true contact, when the band is entirely lost to vision. Now, it would seem to be implied by the instructions that the moment to be noted was that of the first appearance of this darkening at egress, and, at ingress, that when it was about to disappear. To show the danger of such an interpretation I may advert to the observation of the Transit of 1874 made at Windsor, New South Wales, by Tebbutt, and published in the *Astronomische Nachrichten*, vol. lxxxv. s. 174.

It is evident from Mr. Tebbutt's description that he failed to observe either contact through waiting at ingress until almost every vestige of the ligament had disappeared, and at egress by endeavouring to catch its first formation.

III. "Si, les bords restant sans déformation, il se produit un obscurcissement du filet lumineux, sans que l'ombre devienne jamais aussi noire que le corps de la planète, l'observateur notera l'instant du contact géométrique. Il devra noter de plus l'instant de la formation ou de la disparition de l'ombre."

I only remark on the last sentence of this, that, as already explained, there can be no precise moment assigned at which the shade commences, nor do I clearly see how it can disappear otherwise than by becoming as dark as the body of the planet, a case supposed not to exist in the preceding sentence.

IV. "Si l'ombre interposée est d'abord ou devient aussi noire que le corps de la planète, l'instant défini précédemment est celui où cette égalité cesse ou celui où elle s'établit."

This definition of the time of contact concurs with the results of observations on the artificial Transit. But there seems room for question whether observers will in all cases be able to distinguish between this and the phenomenon described in II. It might be inferred, from the independence of the several definitions, that the Conference considered the phenomena of "l'ombre" and the "black drop" or "ligament" as distinct and separable. But there may be room for question whether this view can be sustained, and whether what is understood by the shadow is not simply the commencement or termination of what, when it becomes sufficiently marked, will be described as the black drop. Without, however, directly discussing this and the collateral question, I shall briefly indicate my views of the principles involved.

I. The great diversity of descriptions given by different observers arises as much from different ways of considering the phenomena as from differences among the actual phenomena. As a matter of fact, the character of the appearance is continually changing through the influence of atmospheric undulations, and one observer will catch and describe one appearance and one another. It is therefore essential to avoid merely subjective differences, that observers should have a clear idea of what they are to look for.

II. Assuming that the observations to be finally discussed will be made with sufficient optical power and with good

instruments all set to focus, there will be but two principal sources of difference between actual phenomena. These are:—

a. Different degrees of atmospheric tremors or blurring.

b. Different degrees of contrast between the brilliancy of the Sun's limb and that of the atmosphere immediately adjoining it. The greater the irradiation produced by atmospheric tremors, and the greater the contrast between the brilliancy of the Sun and sky, the more decided may we expect to see that form of distortion commonly described as the black drop. In order to make all the observations comparable, we must seek out some phenomenon which shall be independent of these various conditions. This seems difficult unless the observer has himself a knowledge of the general theory of the subject so that he will know how to make the best estimate under any conditions.

III. Observations of the artificial Transit, made so as to imitate as nearly as possible the appearance of the actual Transit, seem to indicate that the only moment which remains nearly the same under all circumstances is to be estimated by confining the attention to the cusps of light and the general outline of the Sun and planet near the time of contact. If the contrast between the brilliancy of the Sun and sky is considerable, the moment of true contact at ingress is that when light is about to glimmer all the way across the dark space between the cusps. The first glimmer may, however, be a fine line of light, a general haziness, or a confused mixture of luminous filaments covering a considerable breath, according to the distinctness of vision and the steadiness of the atmosphere. Very generally there will be a period of several seconds during which the light will glimmer and disappear by turns. The middle of this period is that of the true contact.

IV. If, owing to haze, smoke, or clouds, the contrast between the brilliancy of the Sun and of the sky is much less than usual, it may be that the actual completion of the thread of light cannot be noted for some seconds after true contact at ingress, but the practised observer will still be able, by estimation, to say when the cusps should have met had they been brilliant and therefore sharply defined. The appearance to be noted is not that of supposed geometrical tangency, a moment which cannot be estimated with any precision whatever, and is substantially valueless, but a moment defined by a general estimate for which no precise directions can be given, and of which the eye itself is a good judge. If in lieu of the first sentence of Article V. of the instructions we should direct the observer to prolong the cusps around *Venus* "*par la pensée*," and note the moment when the cusps thus prolonged should meet, it would accord closely with the view here indicated.

V. I should regret to have exterior contacts neglected until we have some better evidence than has yet been furnished that they are not as accurate as interior contacts.

VI. One disturbing circumstance which might frequently

interfere with the estimation of the contact seems to have been overlooked by the Conference. I refer to the bright line of light which encircles that portion of *Venus* outside the Sun's limb when a portion of her disk is projected upon the Sun. This line of light no doubt grows brighter as the moment of internal contact at ingress approaches, and there is great danger of its being mistaken for the Sun's limb about the moment of contact. It does not appear, however, that anything can be done except to caution the observer against this mistake. He must distinguish between this light and that of the true limb of the Sun as best he can.

VII. It is essential to the accuracy of observation that observers should practice on an artificial Transit. To get the best results from these instruments it is necessary that it should be so placed as to imitate as closely as possible the special phenomena of the actual Transit of *Venus*. These phenomena are atmospheric undulations, a softening of the outline of the Sun's disk and the partial illumination of the background.

I have said nothing on the subject of photography, because I cannot, in a small space, add anything of material value to the contents of my paper of 1872 published as Part I. of the papers prepared by the American Commission on the subject. It was there pointed out that only one practicable method had been proposed for an accurate determination of the solar parallax from photographs of the Transit, and I am not aware that the difficulties there pointed out were obviated in any other plan for photographing the Transit. As an example of what may be done under the most unfavourable circumstances, and on a first trial, by the horizontal meridian photoheliograph, reference may be had to Part I. of the American "Transit of Venus Observations."

Alleged Errors in the Time-Record of Observations of the Transit of Venus, at Hermitage, Rodriguez. By Lieut. W. Usborne Moore, R.N.

(*Communicated by the Astronomer Royal.*)

Page 397 of the account of the observations of the Transit of *Venus*, 1874, contains a note, which suggests four mistakes in time at Ingress and four at Egress, both groups of errors being in the same direction. I feel it my duty to record my respectful dissent from this note, feeling sure that no such mistakes were made, and that any calculations based on the assumption that a minute is to be added will be in error.

The Greenwich sidereal times at the principal phases observed at the three Rodriguez stations are recorded in the following table :—

Point Venus.

Phase observed.	Greenwich Sidereal Time. h m s
Circular Contact	7 38 36·9
Change of colour of ligament	7 39 3·9
Disappearance of ligament .	7 39 27·2
Formation of ligament ...	11 8 39·7
Apparent Contact (fig. 2, p. 366)	11 9 57·3

Point Coton.

Phase observed.	Greenwich Sidereal Time. h m s
Ingress.	
Simple Internal Contact... ...	7 38 54·8
Total disappearance of black hazy drop	7 39 31·5
Egress.	
First appearance of brown haze .	11 8 42·2
First appearance of black hazy drop	11 8 50·8
Circular Contact	11 9 29·7

Hermitage.

Phase observed.	Greenwich Sidereal Time. h m s
Phases from my Note-book.	
Circular Contact	7 37 20·2
Broad ligament forming ...	7 37 31·6
Planet's disk apart from limb of Sun	7 38 12·6
Ligament assumed a more definite shape	7 39 23·6
Saw ligament snap	7 39 48·6
Link formed	11 7 46·4
Apparent Internal Circular Contact (fig. 3, p. 395) ...	11 8 37·5
Can see that portion of *Venus* outside the Sun's limb; a dark brown colour, with limb slightly illuminated (fig. 4, p. 395).	11 10 50·5

It is no part of my business to reconcile the times shown here to be at variance, but rather to demonstrate briefly the improbability of the mistakes imputed. I may, however, point out that the interval of $1^m\ 10^s$ without change of phenomenon, at Ingress, at Hermitage, is no more remarkable than that of $1^m\ 33^s$ during Egress at Suez. The observer remarks (p. 395) that the Sun was too strong for him, and from personal observation I can state that the "boiling," or agitation of the Sun's limb was excessive, owing to the close neighbourhood of a low cloud which was slowly moving, thus changing the conditions of refraction caused by the vapour at its edge. These circumstances may help to account for the interval.

Respecting Egress: it will be noticed that fig. 3, p. 395, illustrates a phase at least a minute previous to fig. 2, p. 366. It is clear that what was Internal Contact to the observer at Point Venus was not Internal Contact to the observer at Hermitage.

It may be considered as some evidence in favour of the correctness of the minutes at Egress that fig. 4, p. 395 (and fig. 145 of the Parliamentary Report), with accompanying description, assuredly indicate a phase not less than $2^m\ 13^s$ after Internal Contact.

It is right to mention that, to the best of my belief, the second and minute hands of the Parkinson and Frodsham chronometer corresponded very nearly; that the second hand had unequal arms, and could not be observed at the wrong side of the dial; that I had been in the habit of using this and similar chronometers for over two years of surveying duty; that during the week previous to the Transit I had taken time for over 200 observations; that comparisons were frequent; that between Ingress and Egress I took 45 observations of the Sun, timing myself: comparisons followed Egress; then noon comparisons, afternoon Sun observations, and night observations of rockets. The watch, in short, was thoroughly familiar. Before Ingress the "minutes" especially were watched in order that the observer might be informed when the contact should occur by Nautical Almanac. The question arises—Is it probable that the watch was read wrong eight times, and only for the critical phenomena, and that the error of one minute had in every case the *same sign*?

[The following is an exact copy of the original record of the observations in Lieut. Hoggan's note book. It will be remarked that in the case of one of the times recorded at Ingress, an alteration had been made in the figures.—W. H. M. Christie.]

Observations of the Transit of Venus, 1874, *at Hermitage, Rodriguez.*

Copy of Lieut. Hoggan's Note-book.

N.B. Until 6.10 clouds obscured the Sun.

About 6.10 first saw the planet on the Sun about $\frac{1}{4}$ on.

Great atmospheric disturbance. Planet and Sun's limb very shaky, using lowest power in the telescope.

Sun obscured by thick clouds.

Cloud cleared away.

h	m	s	
6	21	40	Saw Sun and planet.
	22	21·8	Planet a little more than $\frac{1}{2}$ way on. Great atmospheric disturbance. 2nd power, yellow eyepiece. Venus boiling all round.
6	24	35·2	Cusps as sharp as can possibly be under circumstances. Outside the Sun (of *Venus*) brown colour. Inside the Sun, black.
6	26	30	Boiling seems to increase around the limb of *Venus* and the Sun. Cloud coming up.
6	27	30	A light cloud passing over. Shifted glass to light red.
6	28	0	Cusps still sharp.
6	28	44	Obscured.
6	33	0	Without shade. Cloud partially cleared away. Saw planet.
	33	17·5	Cusps still sharp; not quite circular contact. Put on red-glass eyepiece (No. 2).
	33	48	Just coming on to circular contact.
	33	36·2	Not quite circular contact. (Nearly sure this is 56s.—W. U. M.)
	34	4·8	I conceive this to be circular contact.
	34	16	Black drop forming, broad ligament—or disk of planet well on Sun.
	34	57*	Only ligament, planet disk not joined to Sun. Immense boiling.
	36	7·9	Ligament assumed a more definite shape.
	36	17·4	Ligament getting perceptibly narrower.
	36	32·8	Saw a ligament snap (fade away would be the proper term). I believe this to be internal contact.
	36	43·2	Fancy saw a ligament about as broad as a needle snap. Immediately after planet appeared well on Sun's disk.
	41	0	Sun obscured.

Remarks 15 minutes after :—

The atmospheric disturbance around the Sun's limb and the planet's limb was such as to render it impossible for me, although I watched very carefully, to distinguish the various changes of the black drop with great accuracy.

This I did notice, that the ligament decreased in width without increasing in length very perceptibly. I noticed no

* The figures have been altered in the observer's book. This appears to have been written originally 35·37, and afterwards altered to 34·57.

brown haze whatever at internal contact. Near internal contact the Sun was as bright as could possibly be: so much so that, had there been time to shift, I should have used the darkest shade.

About $\frac{1}{4}$ hour after the planet was on the Sun's disk most of the atmospheric disturbance seemed to have ceased.

The weather before sunrise promised exceedingly well. I rose at two o'clock in the

[Here follow comparisons of chronometers.]

Egress.

h	m	s	
10	3	18·2	B. d. forming.
	3	51·0	Light not quite interrupted.
	3	56·0	Link.
	4	47·0	Apparent internal circular contact.
	7	0·0	Can see *Venus's* limb outside Sun's limb. Portion of *Venus* inside the Sun appears black, that on outside appears brown with slight illumination of limb. Highest power used. Yellow glass.
About	33	45	Planet appeared to be just disappearing.
About	34	0	Telescope failed to work. Probable disappearance.

Remarks.

The observation of Egress was made under totally different circumstances from Ingress. There was no atmospheric disturbance whatever. The Sun's limb and *Venus's* limb were both beautifully defined, and the model was a most perfect representation of them.

On the best Mode of undertaking a Discussion of the Observations of Contact to be made at the approaching Transit of Venus. By David Gill, Esq., LL.D., Her Majesty's Astronomer, Cape of Good Hope.

In most discussions of previous Transits of *Venus* the difficulty has been felt that preconceived ideas on the part of those who have made the discussions may have unconsciously influenced the interpretation put upon the language of the observers.

This difficulty can only be obviated by so conducting matters that none of the persons concerned in the discussion can be aware what influence a particular interpretation will have upon the resulting parallax.

With this view I make the following proposals:—

(α) That an International Committee should be appointed whose secretaries or others appointed for the purpose shall receive the observations made by the various observers of the Transit.

(β) That the secretaries or others (who are to have no voice in the after discussion of the results) shall, as soon after the Transit as possible, prepare for publication the descriptions of the observations of contact made by the various observers, precisely in the language of the observers themselves (giving the original language and a translation in French), but without any statement of the names of the observers, of the locality or time of observation—merely bracketing together the successive observations made by the same observer, and dividing the observations into two lists—those of Ingress and those of Egress—and numbering the separate observations 1, 2, 3, &c., for identification. Such a list when published will afford all the data necessary for deciding the "phase" of contact observed, but would afford no information as to the effect which a particular interpretation of any given observation would have upon the resulting parallax.

(γ) On the publication of this list the Committee should be convened for the purpose of selecting all the observations which appear to answer to the given definition of "contact"—or, if necessary, arranging in corresponding groups the various kinds of contact observed. The result of this classification should then be published, but until it has been issued, no one, beyond the secretaries, should have access to the original observations.

(δ) The "phases" having been thus definitively "cast," the computation of the Greenwich mean time of each observation, and the equations which result from the observations, should be made under the direction of the Committee.

[It is here to be understood that the reduction of the time observations, and determinations of longitude have already been made by the observers themselves, or by the heads of the different national expeditions.]

(ϵ) The Committee should then proceed to combine the equations, dividing them into groups according to the phase observed and the class of telescope employed, and ascertain the relative weight of an equation of each group. The definitive value of the parallax would then be found from the combination of the results of each group, having regard to the weights of these results.

Such a discussion would rest on a broad and impartial basis and would command the acceptance of the scientific world.

Réponse à la communication faite à la Société Royale Astronomique par M. Marth, au sujet de mon appareil pour la détermination des flexions. Par M. Loewy, membre de l'Institut.

Monsieur Marth dans la communication * faite à la Société Royale Astronomique de Londres au mois de Décembre vient d'entretenir ce corps savant d'un appareil pour l'étude de la flexion proposé par lui en 1862, et il conclut à la similitude de son instrument avec le mien en ajoutant que les objections faites par moi contre le mode de construction et l'emploi de son appareil manquent de clarté, et sont pour lui d'une nature énigmatique.

Il m'a paru, en effet, superflu d'exposer les inconvénients des dispositions adoptées par M. Marth, parce que ces inconvénients sont tellement considérables et tellement évidents qu'ils doivent immédiatement frapper tout astronome au courant de ce problème si délicat.

Dans de telles conditions, il m'a paru bien préférable de ne pas soumettre à une critique plus détaillée l'appareil de M. Marth.

Je me vois à regret obligé de réfuter complètement toutes les allégations que M. Marth a produites sur cette question dans la séance précitée.

J'exposerai d'abord les raisons qui s'opposent à l'usage de l'appareil préconisé par M. Marth, et j'expliquerai ensuite toute la différence qui existe entre son instrument et le mien.

L'instrument de M. Marth se compose de deux objectifs et d'un miroir percé à son centre. Chaque objectif est lui-même formé de deux pièces (crown et flint) : c'est donc en réalité un appareil de cinq pièces.

Pendant la rotation de la lunette, ce système est soumis à des déplacements multiples : 1° l'appareil dans son ensemble éprouve divers mouvements de translation et de rotation ; 2° par suite du mode d'attache, quel qu'il soit, les diverses pièces donnent lieu à de nombreux mouvements indépendants les uns des autres : c'est ainsi entre-autres que le flint et le crown peuvent se déplacer l'un par rapport à l'autre, car pour ne pas déformer les images, on ne doit pas trop les serrer.

Les tiges, supports ou vis qui relient les deux objectifs et le miroir à la pièce fondamentale ou directement au cube fléchissent d'une façon quelconque sous le poids qu'ils portent pendant la rotation de la lunette et provoquent des déplacements relatifs.

M. Marth ne se préoccupe nullement des mouvements relatifs de ces diverses pièces; il ne considère que quelques-uns des mouvements d'ensemble dont il croit pouvoir déterminer l'effet par le retournement de son appareil ; c'est ainsi qu'après l'avoir enlevé et retourné M. Marth suppose que les déplacements de l'instru-

* M. Loewy refers to some remarks made by Mr. Marth at the December meeting, in the course of the discussion upon a paper by Mr. Stone on a new form of transit circle with a prismatic object-glass.

ment conservent les mêmes valeurs numériques (abstraction faite du signe). Or aucune de ces deux hypothèses n'est admissible, elles ne présentent même pas *a priori* le moindre degré de probabilité, lorsqu'il s'agit d'évaluer des quantités du dernier ordre de petitesse.

Lors des premières études avec mon appareil, d'une construction beaucoup plus simple que celui de M. Marth, M. Perrotin, Directeur de l'observatoire de Nice, MM. Périgaud, Renan, plusieurs autres astronomes de l'observatoire de Paris et moi-même, après avoir complètement résolu le problème par la détermination de toutes les inconnues, nous avons retourné l'appareil pour opérer dans des conditions différentes, et malgré toutes les précautions prises pour le fixer d'une manière identique nous avons constaté que le mouvement de l'instrument dans la position inverse était tout-à-fait différent du premier et qu'il était impossible d'établir aucune relation entre les deux.

Nous avons aussi reconnu que le jeu des vis du réglage provoque des déplacements très-appréciables des images, et nous avons été amené dans la seconde série d'expériences à les supprimer presque toutes et à coller à l'arcanson celles que l'on avait conservées. Dans nos premières expériences, avec le grand cercle méridien nous avons constaté un mouvement de rotation de l'est à l'ouest. Le déplacement était *a priori* si peu probable qu'il nous a fallu plusieurs semaines pour pouvoir trouver une cause physique probable pour l'expliquer (voir mon mémoire publié dans les Annales de l'Observatoire que j'ai adressé à la Société Royale Astronomique).

Il est donc évident que ce qui se présente pour un appareil aussi simple que le mien existe *a fortiori* pour la disposition beaucoup plus compliquée de M. Marth. Lors qu'on veut atteindre la précision la plus élevée, il n'est pas permis de recourir à des procédés hypothétiques, et l'on ne peut rien admettre qui ne soit demontré par des expériences directes.

En résumé, pour étudier la variation d'une ligne de visée unique, pendant la rotation de la lunette, M. Marth admet, contrairement à la réalité, l'invariabilité des cinq lignes de visées différentes provenant des cinq pièces optiques composant son instrument. Aucun de ces inconvénients n'existe dans mon appareil, qui a été conçu dans le but de pouvoir déterminer avec exactitude tous les mouvements susceptibles de se produire pendant la rotation de la lunette, sans avoir besoin de recourir *a priori* à des lois hypothétiques tant sur la nature de plusieurs de ces mouvements que sur l'action négligeable de quelques autres.

Mon appareil actuellement commandé pour plusieurs autres observatoires se compose d'un seul disque de verre assez épais pour pouvoir être collé à l'arcanson dans un barillet, de telle sorte que les deux pièces forment un tout invariable, et tous les déplacements de l'appareil ne peuvent alors provenir que de la pesanteur de cette pièce unique attachée au cube de la lunette. Une fois introduit dans le tube central sur la fibre neutre, cette

pièce y reste dans la même position sans qu'il soit besoin de l'enlever.

Par des dispositions prises d'avance, il est facile d'en doubler ou tripler la pesanteur par l'adjonction de poids supplémentaires symétriquement placés et autant que possible de même forme que l'appareil, et déterminer ainsi par un procédé nouveau les divers mouvements qui peuvent se produire pendant la rotation.

Toutes les méthodes proposées pour l'étude de la flexion ont échoué, comme celle de M. Marth, devant cette difficulté considérable de la détermination des mouvements propres des systèmes imaginés, et sont restés à l'état de simples conceptions théoriques, sans application possible.

Tous mes efforts ont précisément été dirigés vers ce double but: Trouver d'une part une méthode certaine pour la mesure des déplacements, et d'autre part imaginer une disposition d'appareil rendant possible l'emploi d'une rigoureuse méthode d'expérimentation.

J'ai la certitude aujourd'hui d'avoir complètement résolu le problème à ce double point de vue. Ce nouveau moyen d'évaluer les mouvements de l'appareil, comporte des vérifications nombreuses par la faculté même qu'a l'observateur de faire varier les poids.

Comme toutes les images sont produites par le seul disque de verre, on peut en outre déterminer ces mêmes mouvements d'une manière indépendante par trois opérations distinctes:

1° En comparant les résultats obtenus par transparence au moyen des traits tracés sur l'objectif.

2° Par les images réfléchies au moyen de la surface argentée du miroir.

3° Par la division insérée dans le tourillon.

Si l'on double et triple les poids de l'appareil, on arrive ainsi à trouver pour chaque déplacement six valeurs indépendantes, et par suite deux pour les flexions elles-mêmes.

Dans le cas présent, où il s'agit de dégager les observations les plus précises (positions des étoiles fondamentales) de certaines erreurs systématiques, il faut éviter d'introduire des corrections qui ne présentant pas les garanties d'exactitude les plus absolues, auraient pour effet contrairement au but poursuivi, d'affecter les résultats obtenus d'erreurs plus considérables que celles dont on cherchait l'élimination.

Or c'est en cela que réside le caractère dominant de mon appareil: à savoir qu'il offre des moyens de vérifications multiples et indépendants et présentant par cela même toutes les garanties d'exactitude qu'on est en droit d'exiger lorsqu'il s'agit d'évaluer des corrections à la fois si délicates et si importantes.

Beaucoup de mes confrères de l'Institut, plusieurs directeurs d'observatoires, et presque tous les astronomes de l'observatoire de Paris, ont fait des expériences avec mon appareil, et par des opérations dont la durée ne dépassait guère cinq à six minutes ont pu se convaincre de la précision de cet appareil. La flexion

obtenue dans la position horizontale était identique à celle obtenue au moyen des collimateurs par les procédés physiques ordinaires.

En dehors des flexions que M. Marth croit à tort pouvoir évaluer avec son appareil, mon instrument permet encore de déterminer la flexion de l'axe central et la forme des tourillons. On comprendra maintenant toute la différence qui existe entre les deux appareils : avec le mien, on mesure avec la plus haute précision et de plusieurs manières, non-seulement la flexion de l'objectif et la flexion de l'oculaire, mais aussi la flexion absolue de l'axe instrumentale et la forme des tourillons, ainsi que tous les mouvements de l'appareil.

Avec l'autre, au contraire, en admettant, ce qui en réalité est impossible, que l'on puisse évaluer les mouvements de l'appareil, on n'obtiendrait que les deux premières données du problème ; c'est-à-dire la flexion de l'objectif, et la flexion de l'oculaire.

On some Results obtained from the Meridian Observations of Mars at the Opposition of 1877. By E. J. Stone, M.A., F.R.S.

A very large number of meridian observations of *Mars* and comparison stars near the planet were made during the Opposition of 1877, at the principal Southern Observatories. The co-operation of the Northern Observatories was not so general as could have been wished ; but a valuable series of corresponding observations were secured at Leyden and Washington.

The results of the observations were forwarded, by arrangement, to the Washington Observatory for discussion. A value of the solar parallax has been recently deduced from a discussion of these observations by Professor Eastman, an assistant at the Washington Observatory, and the results have been published as Appendix III, "Washington Observations," 1877. The value of the solar parallax thus found is

$$8''\cdot953 \pm 0''\cdot019.$$

This value has been deduced solely from a comparison of the North Polar Distances of *Mars*, which were made within a few hours of each other. The method has therefore the advantage of practically avoiding the necessity for any correction of errors of tables ; but this advantage is dearly purchased by the loss of a large number of valuable observations, unless a very considerable number of corresponding observations should be found available. The separate results obtained by Professor Eastman were—

From Washington and Melbourne	8″·9712	± 0″·0316
Washington and Cape...	8·8960	0·0725
Washington and Sydney	8·8846	0·0546
Leyden and Melbourne	8·9693	0·0260.

These separate results are accordant within their probable errors.

For some reasons, which are unassigned, no direct comparisons appear to have been made between the Leyden, the Cape, and Sydney Observations.

In addition to the Leyden and Washington observations, some observations of the Transits of *Mars* and the comparison stars were made over oblique systems of wires at Cambridge, United States; but it is very doubtful whether these observations, made by a method essentially different from that adopted at the other stations, can be properly combined with the other observations. These Cambridge observations, combined with the Melbourne observations, have given Professor Eastman a value of the solar parallax of

$$9''{\cdot}1382 \pm 0''{\cdot}0496.$$

The results which would be obtained by a combination of these Cambridge observations with the corresponding observations made at the Cape and Sydney would not differ greatly from this value; but, for the reason given, it is hardly necessary to complete this part of the investigation.

There has unfortunately been a considerable delay in bringing forward the results obtained from these *Mars* observations; but I did not feel myself justified in discussing these observations before the publication of Professor Eastman's paper, and I should have been exceedingly glad if I could have regarded Professor Eastman's discussion as exhaustive. But of the thirty-eight observations of *Mars* which were made in 1877 at the Cape only nine are included in the discussion, and two of the results are rejected on account of supposed defects in the Cape observations; whilst of forty-four Sydney observations of *Mars* only eleven have been included in the discussion, and the results have in four cases been rejected as defective.

The rejection of the Cape observations of September 1 and September 3 is certainly, in my opinion, quite uncalled for; and I consider that the value of these observations cannot properly be estimated without a consideration of the residual errors of *all* the observations of *Mars* made at the different stations, and the consideration of what changes in these residuals would be introduced by any considerable alteration of the adopted value of the solar parallax. This work I have done, so far as the materials are at present available to me, and I hope to complete the work.

But I have met with a difficulty which renders me unable, at

present, to include the Sydney observations in the discussion. In the original scheme of observations it was recommended that the plan adopted by Dr. Winnecke in 1862 should be followed :—

"Two threads of equal size are inserted in the moveable declination system of the field of the telescope, the distance between them being 3″ or 4″ less than the minimum diameter of the planet during the proposed period of observation. The observation is effected by moving these threads until the two small segments of the planet outside of the threads are seen to be exactly equal. If the thread nearest the micrometer head is designated as thread *a* and the other as thread *b*, then the comparison stars should be observed by bisecting the first, fourth, fifth, and eighth stars with thread *a*, and the second, third, sixth, and seventh stars with thread *b*. This order should be reversed on alternate nights, but in all cases the thread used in bisecting ought to be carefully recorded for each star."

Now, on page 15 of Professor Eastman's paper it is stated as follows :—

"Sydney.

"The observations at Sydney were made with Simm's Transit Circle by Government Astronomer H. C. Russell and Assistant H. A. Lenehan, and according to the method proposed in the circular. They also made a separate observation of *Mars* by bisecting 'with the wire used for the Nadir observation, at the reading zero, *before* the planet came on to the Transit wires.' These extra observations differed so widely from the others that they have not been used."

The Sydney observations of *Mars* and stars are given on pages 31 to 36 of the Sydney Observations, 1877–78. The only information respecting these observations which is available to me at present is on page 6 of the Introduction, where it is stated :—

"The declination micrometer has only one wire, and is fitted with a head counter similar to that on the collimation micrometer. For the purpose of observing *Mars* a second wire was put in 19″·68 from the zero wire, but as it was found to be somewhat in the way when taking the Nadir readings it was removed."

Now, the following is a specimen of a day's work as given in the Sydney volume 1877–1878.

1877, *September* 2.

Name.	R.A.	N.P.D
70 Aquarii	22 42 5·69	101 11 55·56
74 Aquarii	—	102 15 51·66
B.A.C. 8004	—	103 43 24·65
Mars	—	101 56 1·60 *a*
Mars	22 12 21·37	101 56 13·49 *β*
B.A.C. 8199	23 25 53·62	102 12 57·59
B.A.C. 8266	23 40 59·33	102 35 2·97

No intimation is here given of the stars which are bisected by the wires a, b, respectively. And unless great care is taken to balance the observations of stars made with each wire there is always a suspicion of the possibility of systematic error in this method. It is not stated whether the observations of *Mars* are or are not corrected for defective illumination. And there appears to me a considerable difficulty in determining which of the two observations of *Mars* (α) or (β) is really that made by the method of equal segments. Professor Eastman has taken the observation (α) with N.P.D.$=101°\ 56'\ 1''{\cdot}60$ as that made by the method of equal segments, and β with N.P.D.$=101°\ 56'\ 13''{\cdot}49$ as that made by simple bisection; and the observations β have been rejected from the discordance of the results.

I presume that Professor Eastman has had direct information on the point from Mr. Russell, and that the course which he has followed is correct. But I must confess that I should hardly have come to this conclusion from the mere observations, as recorded in the Sydney volume 1877–1878.

It would almost appear from these observations, as recorded, that the stars have been observed on, or reduced to, the principal wire, and that (α) represents the attempt to fix the centre of *Mars* by "bisecting with the wire in ordinary use, before the planet came to the transit wires." In this case the reading β, or $101°\ 56'\ 13''{\cdot}49$, would represent the principal observations of *Mars* made by the method of equal segments; but, supposing it recorded as observed, it would require a correction of $9''{\cdot}84$, which corresponds to half the distance between the parallel wires a and b. This would make the two results α and β agree fairly with each other. On the other hand, if Professor Eastman is right in his selection of the principal observations—and I presume that such must be the case—then, undoubtedly, there is something wrong in the observations as recorded, and some confusion in reducing the observations to a common wire, for it is quite impossible that a constant error of about $10''$ can have been made in bisecting such a planet as *Mars* even by a direct attempt to find the centre.

I have worked up these Sydney observations; but it appeared to me not desirable to give them until the points mentioned have been cleared up.

The results contained in the Table have been obtained as follows:—

I have first computed star-corrections for all the observations of the comparison stars selected for observation with *Mars*. With these star-corrections, mean North Polar Distances have been formed. The mean of the separate results for each star for the Cape, Leyden, Melbourne, and Washington, have then been separately found, and the mean of these four separate results adopted as the North Polar Distance of the star for the purposes of this discussion.

In the determination of a value of the solar parallax from a discussion of observations like those under consideration, the tabular places of the stars are entirely eliminated when the same stars are observed at all the stations. The course now adopted allows of the exhibition of the results on days when some of the comparison stars are wanting, without the introduction of any important systematic errors; and, if desired, the errors can be exhibited as corrections to the final result. The mean of the differences between the mean North Polar Distances of the comparison stars and the adopted mean North Polar Distances is then found for each day at each station, and these mean differences are considered as index corrections applicable to the North Polar Distance of *Mars* on the same day for the station. The tabular North Polar Distances have been, for the present purpose, directly interpolated from the *Nautical Almanac*, and the value $8''{\cdot}95$ has been adopted as the value of the horizontal equatoreal solar parallax.

The "error of the tables" is the excess of the observed North Polar Distance over the tabular North Polar Distance.

These errors have been laid down as ordinates, of which the corresponding times are the abscissæ; and a curve has been swept amongst the points laid down. The corrections for "errors of tables" are the corrections read off from this curve. The residual errors are the excesses of the tabular errors over those read off from the curve.

The residual errors, therefore, represent the real errors on the assumption of the accuracy of the mean curve of errors. It will be seen that the residual errors are small for all the stations. And there appears no strongly-marked preponderance of positive or negative signs between the results for the Northern and Southern Observatories over any considerable portion of the curve. This is the only important point so far as a determination of the Solar Parallax is concerned.

The mean residual errors are very small indeed for all the stations.

Day.	*Cape.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Melbourne.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Leyden.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Washington.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	Day.
	″	″	″	″	″	″	″	″	″	″	″		
July 20							2·74	2·52	+0·22				July 20
21				2·65	2·54	+0·11	2·10	2·56	−0·46				21
22				3·26	2·60	+0·66							22
23				2·69	2·66	+0·03	2·45	2·69	−0·24				23
24													24
25													25
26													26
27				2·78	2·87	−0·09							27
28				3·66	2·93	+0·73							28
29				2·69	2·96	−0·27				3·52	3·03	+0·49	29
30				3 30	3·03	+0·27	2·81	3·04	−0·23	3·50	3·06	+0·44	30
31													31
Aug. 1	3·00	3·13	−0·13										Aug. 1
2	3·46	3·16	+0·30										2
3	2·21	3·20	−0·99	2·83	3·17	−0·34							3
4													4
5				3·56	3·26	+0·30							5
6							3·73	3·33	+0·40	3·26	3·35	−0·09	6
7	3·75	3·35	+0·40	2·97	33·3	−0·36							7

Day	*Cape.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Melbourne.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Leyden.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Washington.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	Day.
	"	"	"	"	"	"	"	"	"	"	"	"	
Aug. 8				2·92	3·37	−0·45	2·92	3·39	−0·47				Aug. 8
9	3·76	3·43	+0·33	4·27	3·42	+0·85							9
10				3·10	3·44	−0·34							10
11				2·89	3·47	−0·58							11
12				3·07	3·52	−0·45							12
13	2·96	3·56	−0·60	4·35	3·55	+0·80							13
14	4·02	3·58	+0·44	3·50	3·57	−0·07							14
15	3·87	3·62	+0·25	4·26	3·60	+0·66	3·39	3·62	−0·23				15
16				4·31	3·63	+0·68							16
17				3·60	3·65	−0·05	3·89	3·66	+0·23				17
18	3·32	3·68	−0·36							3·61	3·70	−0·09	18
19													19
20	3·24	3·73	−0·49										20
21				2·63	3·73	−1·10	3·74	3·74	0·00	1·20	3·75	−2 55	21
22							3·63	3·76	−0·13				22
23				3·84	3·76	+0·08							23
24				3·49	3·77	−0·28							24
25				4·54	3·79	+0·75							25
26				4·68	3·80	+0·88							26

Day.	*Cape.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Melbourne.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Leyden.* Errors of Tables.	Corr. for Errors of Tables	Residual Errors.	*Washington.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	Day.
	″	″	″	″	″	″	″		″	″	″	″	
Aug. 27				3·93	3·80	+0·13				3·49	3·80	−0·31	Aug. 27
28				2·72	3·80	−1·08				3·73	3·79	−0·06	28
29	2·86	3·78	−0·92	3·21	3·79	−0·58							29
30	3·67	3·77	−0·10	4·10	3·78	+0·32	3·78	3·77	+0·01				30
31	3·44	3·76	−0·32										31
Sept. 1	2·96	3·74	−0·78	3·47	3·75	−0·28	4·13	3·74	+0·39	5·41	3·73	+1·68	Sept. 1
2				4·33	3·73	+0·60	3·51	3·72	−0·21				2
3	3·87	3·72	+0·15							5·49	3·71	+1·78	3
4	3·95	3·70	+0·25	3·80	3·71	+0·09	3·32	3·70	−0·38				4
5				2·60	3·69	−1·09							5
6	3·83	3·66	+0·17	2·60	3·68	−1·08							6
7				4·10	3·66	+0·44							7
8													8
9													9
10	2·85	3·57	−0·72										10
11	3·35	3·54	−0·19										11
12	4·07	3·52	+0·55										12
13	3·46	3·49	−0·03	4·14	3·50	+0·64							13
14				1·80	3·46	−1·66							14

Day.	*Cape.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Melbourne.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Leyden.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Washington.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	Day.
	″	″	″	″	″	″	″	″	″	″	″	″	
Sept. 15	4·12	3·40	+0·72	3·48	3·43	+0·05				3·23	3·37	−0·14	Sept. 15
16				3·62	3·38	+0·24							16
17	3·50	3·33	+0·17	2·93	3·34	−0·41	2·59	3·33	−0·74				17
18	3·76	3·29	+0·47	3·57	3·31	+0·26							18
19	2·60	3·24	−0·64										19
20	3·46	3·20	+0·26										20
21							3·09	3·15	−0·06	2·74	3·13	−0·39	21
22				2·77	3·13	−0·36							22
23													23
24	2·53	3·03	−0·50	3·09	3·05	+0·04				2·82	2·99	−0·17	24
25	3·05	2·95	+0·10	3·30	2·97	+0·33	2·60	2·95	−0·35				25
26	3·04	2·89	+0·15	2·76	2·92	−0·16				3·15	2·86	+0·29	26
27	2·51	2·83	−0·32	2·47	2·86	−0·39	2·56	2·83	−0 27				27
28	2·32	2·76	−0·44	2·04	2·79	−0·75							28
29	2·86	2·70	+0·16										29
30													30
Oct. 1	2·06	2·56	−0·50	1·73	2·58	−0·85	2·29	2·56	−0·27	2·96	2 54	+0·42	Oct. 1
2	2·07	2·50	−0·43	2·92	2·52	+0·40	2·09	2·50	−0·41	2·14	2·47	−0·33	2
3	2·61	2·44	+0·17	2·39	2·46	−0·07	2·25	2·44	−0·19				3

Day.	*Cape.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Melbourne.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Leyden.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	*Washington.* Errors of Tables.	Corr. for Errors of Tables.	Residual Errors.	Day.
	″	″	″	′	′	″	″	″	″	″	″	″	
Oct. 4	1·14	2·38	−1·24	2·12	2·40	−0·28	2·01	2·38	−0·37				Oct. 4
5	1·52	2·32	−0·80	2·27	2·34	−0·07							5
6	3·20	2·22	+0·98	2·94	2·24	+0·70	2·20	2·22	−0·02	1·62	2·20	−0·58	6
7				1·58	2·18	−0·60							7
8				3·02	2·13	+0·89							
9				2·31	2·06	+0·25	3·03	2·03	+1·00	2·23	2·01	+0·22	9
10				1·78	2·00	−0·22							10
11				2·72	1·94	+0·78							11
12				1·21	1·87	−0·66				1·52	1·83	−0·31	12
13													13
14				2·10	1·76	+0·34	2·63	1·73	+0·90	1·58	1·71	−0·13	14
15				1·71	1·71	0·00	2·14	1·67	+0·47	1·77	1·65	+0·12	15
16				2·51	1·63	+0·88							16
17										2·34	1·55	+0·79	17
18				1·32	1·54	−0·22				1·82	1·50	+0·32	18
19													19
20													20
21				1·28	1·43	−0·15	1·94	1·41	+0·53				21
Oct. 22				1·76	1·33	+0·43							Oct. 22

The results given by this Table appear to me important. The Cape observations began on August 1, and were continued to October 6, and embrace the period when the horizontal equatoreal parallax of *Mars* was 20″.

If we take the corresponding residuals for all four stations during the period August 1 to October 6, we obtain the following results :—

	Southern Stations.		*Northern Stations.*	
Mean Results.	Cape 38 obs.	Melb. 46 obs.	Leyden 19 obs.	Wash. 14 obs.
Residual Error ...	−0″·118	−0″·085	−0″·162	−0″·039
Error of Tables ...	+3·112	+3·261	+3·038	+3·203
Correction applied .	−3·230	−3·346	−3·199	−3·242
Factor of Parallax .	+0·90	+1·07	−2·19	−1·89

If we include the observations during the whole period July 20 to October 22, we obtain the following results :—

	Southern Station.	*Northern Stations.*	
Mean Results.	Melb. 65 obs.	Leyden 27 obs.	Wash. 22 obs.
Residual Error	−0″·011	−0″·033	+0″·064
Error of Tables	+2·990	+2·873	+2·870
Correction applied ...	−3·001	−2·905	−2·806

In both cases these observations admit of no important alteration of the adopted value of the solar parallax, 8″·95 ; and the agreement between the results affords no grounds for suspicion of a difference of personality in the contacts with the upper and lower limbs of *Mars*.

As the observations of *Mars* at the Cape and Melbourne were made by the method of tangents to the limbs, whilst those at Leyden and Washington were made by the method of equal segments, it may be desirable to point out that the results obtained by both methods lead, as might be expected, to sensibly identical results. In 1862, the Pulkova and Cape observations were made by the method of equal segments ; but the Greenwich and Melbourne observations were made by the method of tangents. The Pulkova and Cape observations gave 8″·96 for the value of the solar parallax, whilst Greenwich and Cape gave 8″·92, and Greenwich and Melbourne gave 8″·94.

Unless it can be proved that there is some cause which affects differently the observations of *Mars* at the Northern and Southern stations, these observations and the corresponding results obtained in 1862 would appear to render it impossible that any value of the solar parallax as small as 8″·75 can be

the true value. The weight of the result given by these observations is certainly very great.

I know of only one cause which has yet been assigned as a possible explanation of the large value given by these meridian observations—viz. chromatic dispersion by the atmosphere. There is, however, no difficulty in proving mathematically that the illumination of the upper limb of a planet is increased and not decreased relatively to the lower limb from this cause. This will be clear enough if anyone will merely consider that the red light from the lower limb cannot be strengthened by the yellow, green, and blue lights of greater refrangibility, whilst the red light from the upper limb will be supplemented by light of greater refrangibility from portions of the planet at slightly greater zenith distances. Under any conditions of illumination, therefore, this cause cannot make the observer cut deeper into the upper limb than into the lower. Atmospheric dispersion might, therefore, be appealed to with some slight plausibility as a cause of the small value which has been deduced from the Heliometer measures, but most certainly it is not directly the cause of the large value found from the meridian observations.

Note on Mr. Neison's Paper on the Corrections to Adams's Semi-Diameter of the Moon. By E. J. Stone, M.A., F.R.S.

I have felt great reluctance to continue any longer in our *Notices* a discussion upon these questions of the diameter of the Moon. The question is not, however, simply whether I am right, or Mr. Neison right, upon a point of combined mathematics and arithmetic, but whether the parallactic inequality deduced from a discussion of a valuable series of observations affords, or does not afford, evidence in favour of a certain supposed long inequality in the Moon's longitude. If that long inequality does exist with a coefficient $1''\cdot2$ it is of the utmost importance that its existence should be recognised, and that its changes should be allowed for in our Lunar Theory; but if the inequality does not exist with any such coefficient, it is of even greater importance that its variations should not be allowed to vitiate our results. I have, since the last meeting, re-examined my work, and I find it correct. But before the publication of Mr. Neison's separate results I was unable to offer any possible explanation of the systematic discordances between the results obtained by us. The error into which Mr. Neison has fallen is now, however, clear to me.

The following are the formulæ from which my results have been obtained :—

Let

B = Burckhardt's parallax in seconds of arc.

δA = The correction to B found directly from Prof. Adams's formulæ.

S = Tabular semi-diameter adopted in any year.

x = The mean correction required by Adams's semi-diameter with the factor 0·273114.

Let

$$\frac{\text{Observed diameter} - \text{Tabular diameter}}{\text{Tabular diameter.}} = n.$$

Then

$$n + 1 = \frac{(B + \delta A)\, 0{\cdot}273114 + x}{S},$$

or

$$x = (n + 1 - \frac{B}{S}\, 0{\cdot}273114)\, S - \delta A \,.\, 0{\cdot}273114.$$

Now, in

1853, 1854, 1855,

$$S = B\, 0{\cdot}2725 \left(1 + \frac{1}{360}\right),$$

$$x = \left\{n + 1 - \frac{0{\cdot}273114}{0{\cdot}2725\left(1 + \frac{1}{360}\right)}\right\} S - \delta A\, 0{\cdot}273114,$$

$$x = (n + 0{\cdot}0005233)\, S - \delta A\, 0{\cdot}273114.$$

If we refer to the "Tables containing corrections to be applied to the values of the Moon's equatoreal horizontal parallax given in the *Nautical Almanac* 1840–1856, in order to make them agree with those calculated from Mr. Adams's tables" (*Appendix N.A.* 1856), we can extract numbers which will give us δA. But, on reference to the *Nautical Almanac* 1853, 1854, and 1855, it will be found that the *N.A.* Parallax = B $(1 + \frac{1}{1200})$.

It follows, therefore, that if δN is the correction found in the *Appendix N.A.* 1856,

$$B + \delta A = B\left(1 + \frac{1}{1200}\right) + \delta N,$$

or

$$\delta A = \frac{B}{1200} + \delta N;$$

therefore

$$x = (n + 0{\cdot}0005233)\, S - \delta N \,.\, 0{\cdot}273114 - \frac{B}{1200}\, 0{\cdot}273114.$$

Now, if we neglect the term $\frac{B}{1200}\, 0{\cdot}273114$, or assume that the corrections given in the *Appendix N.A.* 1856, are corrections

to Burckhardt's Parallax, and not to the *N.A.* Parallax, we shall substantially obtain Mr. Neison's results; but these results are clearly wrong to an extent

$$= \frac{B}{1200}\, 0''.273114 = \frac{3420}{1200} \times 0''.273114 = 0''.78$$

very approximately.

To show that the corrections given in the *Appendix N.A.* 1856, are corrections to the adopted parallaxes of the *N.A.*, and not to Burckhardt's tables, it is sufficient to extract the following corrections :—

1852	Dec. 30	$+2''.8$	Corrections to Burckhardt
1852	31	$+2''.6$	
1853	Jan. 1	$-0''.4$	Corrections to Burckhardt $(1 + \frac{1}{1200})$
1853	2	$-0''.3$	

If we apply the correction $0''.78$ to Mr. Neison's results, and compare them with mine given in the *Notices* for December 1881, we obtain as follows. I have changed the signs, so that the quantities given in these tables are the *corrections* to be applied to Adams's semi-diameter to make it agree, in the mean, with the observed horizontal semi-diameters :—

	Neison's corrections to Adams'.		Stone's corrections to Adams'	Neison's results corrected.
	Obs.	"	Obs.	"	"
1851	6	−0·21	7	−0·43	−0·21
1852	3	+0·86	3	+0·71	+0·86
1853	4	+1·73	4	+0·97	+0·95
1854	3	−0·39	3	−1·17	−1·17
1855	3	−0·25	3	−0·94	−1·03
1856	3	−1·75	3	−1·77	−1·75
1857	3	−0·76	3	−0·76	−0·76
1858	5	−1·02	5	−1·02	−1·02

I have no doubt about the substantial accuracy of my results, but the discordances between the outstanding results are now unimportant. These observations make Adams's semi-diameter too large by $0''.51$, but Messrs. Campbell and Neison have assumed, in their work, that this semi-diameter is too small by $0''.18$ for the same group of observations. The rejection by Mr. Neison of the diameters observed by the computers at Greenwich, whilst their observations at Quadratures are retained, is certainly unsatisfactory. With respect to the second point, I have nothing more to remark than that my work is accurate, and that the corrections to the diameter deduced by me refer to the period 1843–1851 under discussion; and the results certainly do not

bear out the inferences which Messrs. Campbell and Neison have made from them. The authors appear to have overlooked the fact that if personal systematic differences exist in the observations of the limbs, it is only the unequal distribution amongst the observers of the observed diameters and observations at Quadratures which affect the final result.

On the Inclination of the Ring of Saturn to its Orbit, deduced from Washington Observations. By Professor Edward S. Holden.

(*Communicated by the Secretaries.*)

The observations given in the following pages were made by Professor A. Hall and myself during the years 1877, 1878, and 1879, which were favourable for the purpose.

The instruments employed were the 26-inch Clark Refractor, crtomethe filar mier, and eyepieces magnifying from 400 to 800 times. The resulting position-angles of the major axis of the ring are given in the fifth column of the table. The reductions were made by the following formulæ, where p is the angle of the northern half of the minor axis of the Ring and the Declination-circle through *Saturn's* centre.

The formulæ are Bessel's, and are given in Engelmann's Bessel's *Abhandlungen*, vol. i. p. 321.

$$\cos a \,.\cos \mathrm{A} = \cos p,$$

$$\cos a \,.\sin \mathrm{A} = \sin p \,.\sin \delta,$$

$$\sin a \quad = \sin p \,.\cos \delta,$$

$$\tan \mathrm{B} \quad = \frac{\tan a}{\cos(n - a + \mathrm{A})},$$

$$\tan \mathrm{C} \quad = \tan(n - a + \mathrm{A}) \frac{\cos \mathrm{B}}{\cos(\mathrm{B} + i)},$$

$$\tan i' \quad = \tan(\mathrm{B} + i) \frac{\cos \mathrm{C}}{\cos(n'' - n' + \mathrm{C})}.$$

In these formulæ a, δ, are the geocentric coordinates of *Saturn*; i is the inclination of *Saturn's* orbit to the Earth's equator, and n the longitude of the node of *Saturn's* orbit on the same plane; n' is the node of the Earth's equator on *Saturn's* orbit, and n'' is the node of the Ring-plane on *Saturn's* orbit, i' being the (required) inclination of these planes.

I have assumed Bessel's values of n, n', n'', and i, since the introduction of Le Verrier's or Hill's values for ☊ and i will not change the results for i'.

The following table gives each observation and the resulting value of i'.

Table giving Washington Observations of Saturn's Ring, and the Computed Values of the Inclinations of the Plane of the Ring to Saturn's Orbit.

No.	Date.	Date.	Wash. M.T.	p	Obs.	Hall. $i'=$	Holden. $i'=$
			h m	°		° ′ ″	° ′ ″
1	June 18	1877·46	15 8	94·82	H.	26 42 35	
2	19	·47	15 20	4·80	H.	26 44 39	
3	22	·47	14 50	5·18	H.	27 4 34	
4	23	·48	14 57	4·76	H.	26 39 23	
5	25	·49	14 46	4·80	H.	26 41 55	
6	July 10	·52	14 50	5·20	Hn.		27 5 14
7	11	·53	14 6	4·68	Hn.		26 40 52
8	11	·53	14 7	4·58	Hn.		26 28 27
9	13	·53	14 0	5·03	Hn.		26 55 19
10	Aug. 3	·60	12 13	5·3	Hn.		27 7 43
11	10	·61	13 38	4·75	H.	26 32 49	
12	11	·61	11 36	4·73	H.	26 59 1	
13	15	·63	12 35	5·01	H.	26 46 55	
14	16	·63	12 54	4·98	H.	26 44 46	
15	17	·63	13 8	4·93	H.	26 41 24	
16	18	·64	12 12	4·98	H.	26 44 10	
17	27	·66	13 34	4·93	H.	26 38 0	
18	28	·66	13 13	4·78	H.	26 28 40	
19	Sept. 23	·73	10 44	5·13	H.	26 40 12	
20	29	·75	9 40	5·08	H.	26 35 9	
21	Oct. 1	·75	10 9	5·18	H.	26 40 36	
22	2	·75	10 28	5·00	H.	26 29 22	
23	5	·76	7 17	5·27	Hn.		26 44 46
24	9	·78	9 24	5·75	Hn.		27 12 30
25	13	·79	10 47	5·77	Hn.		27 11 35
26	13	·79	9 19	5·18	H.	27 6 40	
27	14	·79	8 25	5·30	H.	26 47 38	
28	14	·79	9 59	5·56	Hn.		26 58 44
29	15	·79	11 12	5·68	Hn.		27 6 45
30	16	·79	10 39	5·71	Hn.		27 8 20
31	17	·80	7 20	5·09	H.	26 30 40	
32	18	·80	6 56	5·26	H.	26 40 40	
33	18	·80	9 49	5·66	Hn.		27 4 49
34	23	·81	7 5	5·21	H.	26 36 33	
35	24	·81	7 35	5·29	H.	26 41 25	

No.	Date.	Date.	Wash. M.T.	p	Obs.	Hall. $i'=$	Holden. $i'=$
			h m	°		° ′ ″	° ′ ″
36	Dec. 2	1877·93	6 26	5·10	H.	26 28 38	
37	7	·94	6 48	5·40	H.	26 47 18	
38	8	·94	6 9	5·15	H.	26 32 30	
39	9	·94	6 18	5·27	H.	26 39 56	
40	15	·96	7 2	4·80	H.	26 12 56	
41	19	·97	6 52	5·20	H.	26 37 56	
42	31	·99	6 23	5·55	H.	27 2 44	
43	Jan. 3	1878·01	6 12	5·50	H.	27 0 44	
44	5	·01	6 18	4·95	H.	26 28 27	
45	12	·03	6 4	5·17	H.	26 44 30	
46	16	·04	6 6	4·90	H.	26 30 2	
47	17	·05	6 10	4·97	H.	26 34 40	
48	19	·05	5 51	4·83	H.	26 27 10	
49	Sept. 9	1878·69	10 30	4·5	Hn.		27 15 52
50	17	·71	10 40	4·1	Hn.		26 52 41
51	19	·72	10 50	3·9	Hn.		26 40 55
52	Oct. 1	·75	11 30	4·5	Hn.		27 11 8
53	8	·77	11 0	4·4	Hn.		26 59 19
54	14	·79	9 30	3·9	Hn.		26 29 14
55	21	·81	8 30	4·6	Hn.		27 6 7
56	25	·82	9 18	4·5	Hn.		26 59 56
57	26	·82	9 37	4·2	Hn.		26 41 55
58	Nov. 5	·85	10 44	4·5	Hn.		26 59 35
59	13	·87	8 33	3·60	H.	26 12 52	
60	13	·87	9 42	4·2	Hn.		26 36 56
61	14	·87	7 50	3·90	H.	26 18 50	
62	14	·87	8 53	4·3	Hn.		26 42 55
63	15	·88	7 30	3·85	H.	26 15 45	
64	18	·89	9 13	4·23	H.	26 38 29	
65	20	·89	8 23	4·2	Hn.		26 36 41
66	Oct. 16	1879·80	11 5	3·4	Hn.		27 11 30
67	20	·81	9 48	3·3	Hn.		27 8 34
68	23	·81	12 0	3·2	Hn.		27 4 40
69	24	·82	8 48	3·3	Hn.		27 11 35
70	25	·82	9 47	3·2	Hn.		27 6 7
71	26	·82	8 38	3·4	Hn.		27 19 15
72	27	·82	12 5	3·2	Hn.		27 7 45
73	28	·83	10 0	3·1	Hn.		27 2 11

No.	Date.	Date.	Wash. M.T.	p	Obs.	Hall. $i'=$	Holden. $i'=$
			h m	°		° ′ ″	° ′ ″
74	30	·83	10 6	3·3	Hn.		26 59 41
75	31	·83	10 21	3·1	Hn.		26 46 55
76	Nov. 11	·87	9 49	3·5	Hn.		27 18 40
77	22	1879·89	8 36	93·3	Hn.		26 55 2

From the 40 observations of Professor Hall we have for

$$1877{\cdot}948 = 1877,\ \text{Dec. } 11,$$
$$i' = 26^{\circ}\ 38'\ 47'' \pm 1'{\cdot}4.$$

The probable error of a single observation is $\pm 8'{\cdot}6$.

From the 37 observations made by me, we have for

$$1878{\cdot}771 = 1878,\ \text{Oct. } 8,$$
$$i' = 26^{\circ}\ 57'\ 2'' \pm 1'{\cdot}7.$$

The probable error of a single observation is $\pm 10'{\cdot}3$.

Bessel's 22 observations give for

$$1818{\cdot}726 = 1818,\ \text{Sept. } 20,$$
$$i' = 27^{\circ}\ 0'\ 9'' \pm 5'{\cdot}2.$$

The probable error of a single observation was $\pm 24'{\cdot}1$.

I have not combined the results obtained by Professor Hall and myself, since the observations of 1877, October 13 and 14, and of 1878, Nov. 13 and 14, show that they are not strictly comparable. My result agrees with Bessel's within the limits of the probable errors. A comparison of the results of Professor Hall and of Bessel would indicate a diminution of the inclination since 1818.

Washburn Observatory,
University of Wisconsin, Madison:
1882, *January.*

Postscript.—Since submitting the above to Professor Hall, I have found two observations of the position-angle of *Saturn's* Ring made by Dr. Auwers in 1861 with Königsberg Heliometer in the publications of that Observatory for 1865, page 132. They are

1861, April 15; $p = 83^{\circ}\ 14'$ (2),
23; $= 82\ 39$ (2).

These, when reduced as above, give

		° ′
1861·29	i' =	26 48·6
1861·31	i' =	27 56·0 or
1861·30	i' =	26 52·3 (2 nights.)

1882, *March.*

Conjunctions of the Interior Satellites of Saturn.
By Professor Asaph Hall.

Date.	Wash. M. T. h m	Satellite.	Conjunction.	Remarks.
1881, Sept. 19	13 35·8	Enceladus	Superior	
Oct. 22	10 39·3	„	„	
Nov. 14	9 45·1	Dione	Inferior	Poor
15	10 50·4	Tethys	„	Good
16	9 29·7	„	Superior	„
28	10 12·8	Enceladus	„	Faint
Dec. 8	10 27·9	Rhea	„	Good
10	9 42·5	Dione	„	
1882, Jan. 11	7 25·6	Rhea	Inferior	
27	7 14·1	Dione	„	

Note.—The above observations were made by setting the wire of the micrometer at the zero of position, so as to observe the time of the conjunction in Right Ascension. On November 16, *Mimas* was noticed to be very bright, and near the following elongation at $9^h\ 37^m$ m. t.

U.S. Naval Observatory,
Washington:
1882, *March* 6.

Errata in the First Melbourne General Catalogue of Stars.
By R. L. J. Ellery, Esq., F.R.S.

In Mr. Downing's paper on the N.P.D.'s of the Cape Catalogue for 1880, which appeared in the *Monthly Notices* of the R. A. S. for November 1881, he refers to a discrepancy between the N.P.D. of Lacaille 4342 as given in the Cape and Melbourne Catalogues, and suggests that it might be partly due to proper motion. The difference, however, is explained by a typographical error in the Melbourne Catalogue, where 3 has been printed

instead of 8, so that the seconds ought to be 38·83 instead of 33·83.

Our observations, extending over a period of fifteen years, show no appreciable motion in the P.D. of this star.

I append a list of the errata of the Melbourne First General Catalogue detected up to the present time.

No.	Place.	*For*	*Read*
74	d'	6·2865	1·2865
80	a'	6·6287	9·6287
194	P″	·0034	·0008
347	N.P.D.	13·81	5·02
358	Name	Majoris	Minoris
511	N.P.D.	33·83	38·83
552	P′	·2099	·1485
	p'	·033	·007
796	N.P.D.	10·10	40·10
	P	5·833	5·834
	a	9·0054	9·0056
	b	9·1710	9·1712
	c	0·7659	0·7660
	d	8·9732	8·9734
	m	(−0·25)	(+0·01)
	a'	9·7822	9·7823
895	P′	·8356	·7589
910	R.A.	42·95	42·97
	M.	−0·004	0·000
	N.P.D.	58·70	57·48
	m	+0·23	0·00
947	N.P.D.	3d	2d
1067	P	102·365	102·372

The Observatory, Melbourne:
1882, *January* 31.

The Variable Star β Ursæ Minoris. By T. E. Espin, Esq., B.A.

This star has been frequently suspected of variation. Herschel, Struve, and Heis have each observed alterations in its light. The star was seen of unusual brilliancy on the evening of November 5, 1881, and observations were at once commenced which soon showed signs of a period of about ten days. The observations were discontinued of necessity at the end of Decem-

ber, but were resumed again in March 1882. The total number of nights on which this star has been observed is 42, namely :—

1881, November	13	1882, March	15
December	7	April	7

The star was near a maximum or minimum on the following nights :—

Near Maximum.	d	h	Near Minimum.	d	h
1881, Nov.	5	0	1881, Nov.	9	11
	15	6		20	6
	25	13		29	6
1882, March	3	8	Dec.	21	6
	13	8	1882, March	5	8
April	3	11	April	6	10

The star was also observed to be at a maximum on the evening of March 11, 1879. The following elements have been found to represent the observations very satisfactorily :—

Period $10^{d}\cdot6747$

Variation from 2·2 to 2·8

Epoch of Maximum, 1882, April $4^{d}\cdot10$.

The star's magnitude, for the greater part of its period, is 2·5 or 2·6. The minimum takes place two or three days after the maximum. Observations were also made by Mr. T. Read in November and December, which were found to agree with my own. The stars used as comparison stars were :—

α Ursæ Minoris	Mag. 2·4
γ „ „	3·2

The determinations were made with an opera-glass, the stars being placed slightly out of focus. The colour of *β Ursæ Minoris* is usually a bright yellow ; it probably becomes redder at minimum.

Orbits of Meteor-streams, deduced from Observations made during the years 1871–1880 *in Hungary.* By M. R. de Kövesligethy.

(*Communicated by Dr. N. de Konkoly.*)

For some years past observations of meteors have been made in Hungary, especially at O-Gyalla, Selmec, Zágráb, Szathmár-Némethy, Hódmezö-Vásárhely and Gyulafehérvár. There was thus a considerable amount of material, about 5,000 meteors

having been observed. The number of those, however, which were employed in the determination of the radiant points was only 1,088, the greater part of the meteors proving to be scattered, or to have been incorrectly observed. Since 91 radiant points have been deduced, each radiant is derived on an average from 12 meteors.

The calculations have been made with the formulæ published by Dr. Charles Schrader in one of the *O-Gyalla Annales*, which do not differ very much from the approximate method given by Klinkerfues.

Let $\odot$ be the ephemeridical, $\odot'$ the corrected longitude of the Sun, R the radius vector of the Earth. By assumption of parabolical velocity, that is by neglecting $\frac{1}{a}$ in the expression

$$V = \kappa \sqrt{\frac{2}{R} - \frac{1}{a}},$$

where

$$\log \kappa = 8{\cdot}2356,$$

we have the following relations:

$$\cos \eta = \cos b \sin (l - \odot'),$$

where

$$\odot' = \odot + \Delta \odot,$$

and

$$\sin \theta = m \sin \eta,$$

where

$$m = \sqrt{1 - \frac{R}{2}}.$$

$\Delta \odot$ and $\log m$ are to be taken out of the latter following table.

For the inclination we have

$$\tan i = \frac{\sin b}{\sin \eta} \tan (\eta - \theta),$$

and for the anomaly

$$\tan \frac{v}{2} = \frac{\cos i \tan (\eta - \theta)}{\tan (l - \odot') \tan \eta}.$$

Finally,

$$\pi - \Omega = v, \quad \text{and} \quad \Omega = \begin{cases} \odot \\ 180 + \odot \end{cases} \text{ if } b \text{ is } \begin{cases} \text{negative} \\ \text{positive} \end{cases}.$$

The perihelion distance is derived by

$$q = R \cos^2 \frac{v}{2}.$$

As a control I used

$$\cos\frac{v}{2}=\frac{\cos(\eta-\theta)}{\cos i}.$$

The motion is direct, if

$$\eta-\theta<90^\circ,$$

retrogade, if

$$\eta-\theta>90^\circ.$$

TABLE.

With the argument $\odot$ *we get* $\Delta\odot$ *and log m.*

$\odot$	$\Delta\odot$	log. m	$\odot$	$\Delta\odot$	log. m
0°	−0·93°	9·8502	190°	+0·95°	9·8495
10	0·97	8495	200	0·95	8501
20	0·95	8489	210	0·92	8507
30	0·92	8483	220	0·87	8512
40	0·85	8477	230	0·77	8517
50	0·75	8472	240	0·65	8522
60	0·63	8467	250	0·52	8525
70	0·50	8463	260	0·35	8528
80	0·35	8460	270	0·18	8529
90	0·18	8459	280	0·02	8530
100	0·02	8458	290	−0·17	8530
110	+0·15	8459	300	0·33	8529
120	0·32	8460	310	0·48	8526
130	0·47	8463	320	0·62	8522
140	0·62	8467	330	0·75	8518
150	0·73	8471	340	0·85	8513
160	0·83	8476	350	0·88	8508
170	0·90	8482	360	−0·90	9·8502
180	+0·93	9·8488			

The perihelia have been excluded as being of no importance for comparison.

Dr. Edmund Weiss has given a table of the comets which nearly approach the Earth's orbit. I have changed this table only by adding the elements of these comets, which are based on the Olbers-Galle Catalogue. The second column gives the comet and node in which the approximation happens, the third the time of this approximation, the fourth the distance from the Earth's orbit, and the fifth the radiant point in longitude and latitude. The data are reduced to the mean equinox of 1850.

Catalogue of Comets which approach near to the Earth's Orbit.

No	Comet.	Time	R−r	l	b	i	π	Ω	q	Motion.
1	1792, II. ♂	Jan. 5	−0·066	182·2	27·9	49·12	135·87	283·23	0·9668	R.
2	1840, I. ♌	20	+0·036	141·1	−45·5	53·08	192·20	119·95	0·6185	D.
3	1718, ♌	29	−0·042	217·6	−18·2	31·13	121·65	127·92	1·0254	R.
4	1857, I, ♂	Feb. 2	−0·028	258·3	46·3	87·93	74·73	313·15	0·7725	D.
5	1092, ♌	5	−0·012	110·1	−56·9	88·92	156·33	125·67	0·9281	D.
6	1854, IV. ♂	13	+0·015	320·7	55·0	40·90	94·40	324·47	0·7987	D.
7	1858, IV. ♂	13	+0·045	272·2	35·2	80·03	226·10	324 97	0·5543	R.
8	1862, IV. ♂	Mar. 16	+0·013	247·9	22·9	42·47	125·18	355·77	0·8032	R.
9	1683, ♌	16	−0·052	224·0	−34·5	83·78	86·52	173·28	0·5533	R.
10	1763, ♂	18	−0·026	322·4	37·7	72·57	84·95	356·28	0·4983	D.
11	1861, I. ♂	April 20	+0·002	270·6	57·0	79·75	243·37	29·92	0·9207	D.
12	1790, III. ♂	24	−0·063	328·5	33·1	63·58	274·95	35·23	0·7910	R.
13	1863, II. ♌	June 2	−0·054	338·9	−40·3	67·37	247·25	251·27	1·0682	R.
14	1684, ♌	22	−0·010	40·1	−65·8	65·80	238·87	268·25	0·9602	D.

No.	Comet.	Time.	R−r	l	b	i	π	Ω	q	Motion.
15	1850, I. ♂	24	−0·065	1·9	70·5	68·18	272·42	92·88	1·0815	D.
16	1864, II. ♂	27	+0·047	13·5	1·1	1·87	304·22	95·20	0·9093	R.
17	1737, II. ♂	July 29	+0·025	129·2	58·8	39·23	262·60	123·88	0·8670	D.
18	1852, II. ♂	Aug. 10	−0·013	33·6	−27·7	49·18	278·70	317·48	0·9129	D.
19	1862, II. ♂	19	−0·027	48·6	− 4·6	7·90	299·33	326·53	0·9813	R.
20	1854, III. ♂	Sept. 10	−0·018	45·8	−33·7	71·32	273·68	347·65	0·6481	R.
21	1790, I. ♂	16	−0·053	104·7	15·2	29·73	58·40	172·83	0·7473	R.
22	1763, ♂	20	+0·029	33·2	−39·0	72·57	84·95	356·28	0·4983	D.
23	1864, IV. ♂	Oct. 16	−0·044	185·6	50·1	48·87	321·70	203·22	0·7709	D.
24	1779, ♂	19	+0·022	24·5	−42·3	32·52	87·23	25·07	0·7132	D.
25	1849, I. ♂	29	−0·027	147·2	55·1	85·05	63·23	215·20	0·9597	D.
26	Biela ♂	Nov. 28	+0·011	38·7	30·6	12·55	109·13	245·85	0·8606	D.
27	1819, IV ♂	Dec. 9	+0·086	327·9	−35·2	9·02	67·32	77·23	0·8926	D.
28	1680, ♂	26	+0·050	128·6	3·4	60·67	262 82	272·15	0·0062	D.

I consider now the influence of neglecting $\frac{1}{a}$ on the elements.

From

$$V = \kappa \sqrt{\frac{2}{R} - \frac{1}{a}}$$

we find by differentiation

$$dV = \frac{\kappa^2}{2\,a^2 V}\, da;$$

κ^2 being small, and a^2 large, the error in V will be but small. For the November stream, $a = 10{\cdot}340$; for the August stream, $a = 22{\cdot}355$. If we put, for instance, $da = 12{\cdot}015$, we get $dV = 0{\cdot}00015$; we can say, therefore, that, by neglecting the quantity $\frac{1}{a}$ the velocity is hardly altered.

The approximate method follows from the strict one by putting $L = \odot - 90°$, and $R = 1$, where L is defined by the equation

$$\sin(\odot - L) = \frac{0{\cdot}9998}{\sqrt{2\,R - R^2}}.$$

It easily follows that

$$dL = \frac{0{\cdot}9998\,(1-R)\,dR}{(2\,R - R^2)\,\sqrt{2\,R - R^2} - 0{\cdot}9996};$$

therefore, in the most unfavourable case, $dL = 0{\cdot}0144$. The expression for the inclination is

$$\tan i = \frac{\tan \beta}{\sin(\odot - \lambda)},$$

where

$$\tan \beta = \frac{\tan b}{\sin(l - L)} \sin(\lambda - L);$$

l and b are the latitude and longitude of the radiant point, and λ, β, the longitude and latitude of the end point of the tangent drawn to the orbit of the meteor. By substitution and differentiation we have

$$di = \frac{\tan b \,.\, \sin(\odot - \lambda)\, \sin(l - \lambda)\, dL}{\tan^2 b\, \sin^2(\lambda - L) + \sin^2(l - L)\, \sin^2(\odot - \lambda)};$$

and we may put

$$di = \frac{\tan b \sin(\odot - \lambda)\, \sin(l - \lambda)\, dL}{\tan^2 b\, \cos^2(\odot - \lambda) + \cos^2(\odot - l)\, \sin^2(\odot - \lambda)}.$$

If we choose for $\odot$, λ and l the most unfavourable possible values, we get, in the case when $b=89°\cdot17$, an error of $di=1\cdot0000$. In the two extreme cases of our calculation, the least latitude is $-0\cdot66$, and the greatest $=81\cdot12$, giving respectively $di=-0\cdot0002$ and $+0\cdot0922$.

We determine still the error in the anomaly, which is also identical with the error of the longitude of perihelion.

By differentiation of the formula

$$p=\frac{V^2R^2\sin^2\sigma}{\kappa^2}$$

with respect to V, regarding σ as independent from L, an omission of very slight importance, we get

$$dp=\frac{2\,VR^2\sin^2\sigma}{\kappa^2}\,dV,$$

and thus in the worst case, $dp=0\cdot0240$.

From the formula for the anomaly

$$e\cos v=\frac{p}{R}-1$$

we find, regarding p as variable,

$$dv=\frac{-dp}{R\sqrt{e^2-\left(\frac{p}{R}-1\right)^2}};$$

so that the greatest value of dv : $da = -0\cdot1580$. The longitude of the node is determined only by the longitude of the Sun ; the supposition $\frac{1}{a}=0$ has no influence on it.

These few data show sufficiently that the calculation with the approximate formulæ introduces errors which are less than the errors of observation.

In a series of observations there frequently occur meteors which might with equal justice be reckoned as belonging to different radiant points. It is therefore of the greatest importance to note that meteors belonging to different streams are essentially different in colour, in the character of their trains, and in their apparent velocity. It is to be remarked that these differences form the characteristics of the meteor-streams. The Perseids are, for instance, yellow, the trains disappear rapidly, and their brightness continually increases. The following elements are arranged according to the longitude of the radiant point, which allows a better comparison and view. Our purpose is, not to record as many radiant points as possible, but to reduce the many observations to the best determined ones : as, however, among the radiant points from which I have calculated there may possibly still be some uncertainties, I give the following table of identical radiant points and meteor-streams :—

Identical Radiant Points and Meteor Orbits.

Time.	Central No.	No. of Meteors.
1875, July 27·48	2	10
1878, 29·44	3	9
1872, Aug. 12·01	9	11
1875, 10·96	10	10
1874, 9·95	7	10
1872, Aug. 7·42	1	9
1875, 9·99	2	6
1879, July 26·96	1	10
1875, 26·96	1	18
1874, Aug. 12·45	10	7
1872, 10·98	6	9
1872, Aug. 10·98	5	8
1877, 12·93	2	19
1880, July 29·99	9	13
1878, 26·43	1	10
1875, July 28·45	2	23
1876, 27·98	2	9
1873, July 27·99	2	13
1880, 27·95	11	13
1874, Aug. 12·45	9	9
1875, 12·41	12	6
10·96	8	8
12·96	9	14
1871, 9·97	2	7
1878, Apr. 20·94	1	6
1874, 20·63	1	6

Time.	Central No.	No. of Meteors.
1874, Aug. 9·95	6	11
1874, 9·95	5	8
1875, 10·96	7	9
11·95	3	23
1876, 10·92	4	11
1875, Aug. 10·96	5	11
1874, 9·95	3	50
1876, July 27·98	4	14
27·98	1	17
1873, 26·48	1	8
1874, Aug. 9·95	2	9
1876, 13·40	2	6
1876, Aug. 10·92		15
1875, 8·93	1	9
1880, Aug. 9·44	7	22
1875, 10·96	6	14
1872, 10·98	4	8
1874, 9·95	4	25
1879, 11·94	3	12
1875, 11·95	2	12
1874, Aug. 9·95	1	14
1877, 13·44	1	12
1872, 12·42	11	9
1876, 10·92	1	9
1872, Aug. 10·98	8	12
9·97	3	16
1872, Aug. 12·42	13	9
1879, 12·46	5	13
1876, Aug. 10·92	6	16
1872, 12·01	10	22

Time.	Central No.	No. of Meteors	l	b	i	π	Ω	q	Motion.	η	$\eta-\theta$	Station.
1880, July 9·42	10	7	358·98	44·13	15·43	297·41	287·43	1·0093	D.	47·13	16·20	Selmec.
1875, 27·48	2	10	358·03	22·06	56·48	45·15	303·95	0·4091	R.	138·33	110·52	„
1876, 27·98	3	13	357·99	48·04	84·85	247·91	305·17	0·7823	D.	122·00	85·48	O-Gyalla.
1878, 29·44	3	9	357·15	18·16	53·24	60·27	306·03	0·2992	R.	137·36	108·97	„
1872, Aug. 12·01	9	11	356·60	72·17	55·93	295·30	319·50	0·9683	D.	100·49	56·79	„
1880, 9·44	8	8	353·65	37·98	74·50	227·19	317·07	0·5074	D.	117·60	79·10	„
1875, 10·96	10	10	352·72	56·63	63·28	265·99	317·85	0·8195	D.	108·05	66·15	„
1874, 9·95	7	10	352·46	57·36	63·33	266·64	317·10	0·8292	D.	107·96	66·03	„
1875, July 31·98	7	9	347 82	33·20	82·69	209·05	308·27	0 4250	D.	121·86	85·27	„
1872, Aug. 10·98	6	9	344·24	66·19	54·10	282·67	318·53	0·9172	D.	99·87	56·08	„
1875, July 28·45	2	23	335·40	60·02	58·97	258·60	304·92	0·8582	D.	104·50	61·71	„
1873, 27·99	3	10	330·26	15·65	44·79	172·57	305·37	0·1627	D.	113·53	73·49	„
1875, 31·98	5	6	328·62	45·60	52·81	235·71	308·27	0·6595	D.	103·79	60·83	„
1872, Aug. 7·42	1	9	326·10	59·10	46·78	268·66	315·08	0·8564	D.	95·36	51·00	„
1875, July 31·98	6	9	325·71	32·50	45·50	214·37	308·27	0·4729	D.	104·27	61·42	„
Aug. 9·99	2	6	322·50	48·95	40·77	257·76	316·88	0·7670	D.	93·31	48·78	„
1880, 9·44	6	7	320·62	22·43	23·13	230·07	317·07	0·5334	D.	92·75	48·20	„
1879, July 26·96	1	10	319·22	22·96	36·20	196·80	303·52	0·3617	D.	104·08	61·21	„
1875, 26·96	1	18	316·51	12 96	20·96	190·28	303·48	0·3077	D.	102·33	59·07	Zágráb.

Time.	Central No.	No. of Meteors.	l	b		π	Ω	q	Motion.	η	$\eta-\theta$	Station.
1874, Aug. 12·45	10	7	319·96	70·02	41·23	292·74	319·52	0·9584	D.	87·55	42·97	O-Gyalla.
1872, 10·98	5	8	306·45	47·09	29·54	270 33	318·53	0·8444	D.	81·41	37·42	„
1875, July 28·45	1	9	305·10	73·58	44·17	281·68	304·92	0·9740	D.	89·94	45·37	„
1877, Aug. 12·93	2	19	303·47	32·10	19·33	266·71	320·17	0·8080	D.	75·40	32·57	Zágráb.
1876, July 27·98	2	9	302·87	66·54	41·91	272·99	305·17	0·9372	D.	88·93	44·37	O-Gyalla.
1875, 31·98	4	7	296·83	15·23	10·85	241·05	308·27	0·7036	D.	78·54	35·09	„
1878, 29·44	1	9	293·65	53·12	32·51	263·07	306·03	0·8760	D.	82·36	38·30	„
1873, 27·99	2	13	277·21	40 82	20·38	266·37	305·37	0·9018	D.	68·79	27·92	„
1880, 29·99	9	13	270·00	77·55	37·96	294·82	307·10	1·0035	D.	84·46	38·38	Fehérvár.
1878, 26·43	1	10	269 52	73·06	35·98	295·39	303·17	1·0108	D.	80·62	36·82	Selmec.
1877, Aug. 12·93	1	18	269·13	23·76	8·72	296·07	320·17	0·9687	D.	44·13	14·84	Zágráb.
1875, 10·96	9	14	268·25	74·05	34·02	306·17	317·85	1·0028	D.	77·82	34·46	O-Gyalla.
1871, 9·97	2	7	267·27	71·64	32·47	303·83	316·85	1·0001	D.	76·00	33·04	„
1874, April 20·63	2	7	267·11	56·54	78·58	172·14	210·28	0·8980	D.	117·80	79·21	„
1880, July 27·95	11	13	260·17	47·08	19·69	280·38	305·18	0·9689	D.	60·98	23·13	Selmec.
1874, Aug. 12·45	9	9	250·44	81·12	37·60	315·74	319·52	1·0120	D.	81·68	37·64	O-Gyalla.
1878, Apr. 21·44	1	10	245·83	45·93	69·66	138·30	211·10	0·6513	D.	113·89	73 76	Vásárhely.
1875, Aug. 12·41	12	6	244·74	70·88	29·66	314·12	319·18	1·0110	D.	71·56	29·76	O-Gyalla.
10·96	8	8	220·73	73·85	31·52	320·17	317·85	1·0130	D.	74·00	31·54	„

Time.	Central No.	No. of Meteors.	l	b		π	Ω	q	Motion.	η	$\eta-\theta$	Station.
1876, Aug. 13·93	1	10	178·75	63·82	30·24	343·71	321·35	0·9742	D.	74·74	32·07	O-Gyalla.
1878, Apr. 20·94	1	6	174·98	64·25	30·72	187·70	210·60	0·9654	D.	75·68	32·58	,,
1874, 20·63	1	6	168·91	71·69	33·77	198·62	210·28	0·9948	D.	78·24	34·58	,,
1880, Nov. 28·61	12	7	113·26	14·99	56·76	197·23	66·73	0·1728	R.	134·05	103·27	Selmec.
1871, Aug. 9·97	1	9	106·60	62·82	58·25	357·97	316·85	0·8884	D.	103·54	60·48	O-Gyalla.
1874, 9·95	6	11	81·79	42·37	85·35	254·04	317·10	0·7360	R.	127·71	93·96	,,
1876, 10·92	5	15	79·65	53·91	82·76	358·89	318·47	0·8922	D.	120·47	83·21	Vásárhely.
1874, 9·95	5	8	75·80	45·68	85·10	267·80	317·10	0·8370	R.	128·04	94·45	O-Gyalla.
1875, 10·96	7	9	72·15	45·07	81·52	274·59	317·85	0·8758	R.	130·28	97·88	,,
11·95	3	23	64·20	32·45	57·41	281·40	318·80	0·9090	R.	144·65	120·66	Szathmár.
1876, 10·92	4	11	63·33	40·49	70·13	288·07	318·47	0·9434	R.	137·49	109·15	Vásárhely.
1875, 8·93	1	9	62·31	57·24	84·43	335·73	315·83	0·9837	D.	121·36	84·51	O-Gyalla.
10·96	5	11	61·67	46·68	79·40	293·79	317·85	0·9682	R.	131·90	100·37	,,
1874, 9·95	3	50	60·59	47·53	80·57	339·92	317·10	0·9734	R.	131·16	99·24	,,
1880, 9·44	7	22	60·37	36·13	62·64	287·07	317·07	0·9450	R.	141·99	116·36	,,
1875, 10·96	6	14	59·78	32·26	55·90	288·93	317·85	0·9502	R.	146·00	122·87	,,
1872, 10·98	4	8	59·23	39·60	67·43	296·35	318·53	0·9758	R.	129·36	112·13	,,
1874, 9·95	4	25	56·32	39·00	66·14	297·84	317·10	0·9848	R.	140·21	113·50	,,
1879, 11·94	3	12	55·08	36·29	61·09	305·11	318·73	0·9988	R.	143·32	118·51	,,

Time.	Central No.	No. of Meteors.		b		π	Ω		Motion.	η	$\eta - \theta$	Station.
1875, Aug. 11·95	2	12	54·04	32·10	54·29	306·84	318·80	1·0023	R.	147·60	125·49	Szathmár.
10·96	11	15	52·72	66·93	72·71	321·33	317·85	1·0125	D.	113·00	72·72	O-Gyalla.
10·96	4	15	51·43	39·89	85·00	29·17	317·85	0·6690	R.	127·78	94·06	„
1876, 10·92	3	9	49·80	48·40	80·09	317·17	318·47	1·0133	R.	131·60	99·91	Vásárhely.
1874, 9·95	2	9	47·61	33·48	56·28	317·28	317·10	1·0132	R.	146·52	123·72	O-Gyalla.
1876, 13·40	2	6	46·96	32·40	54·78	332·51	320·87	1·0025	R.	147·30	125·00	Vásárhely.
July 27·98	4	14	46·28	66·62	72·64	296·41	305·17	1·0102	D.	112·95	72·69	O-Gyalla.
27·98	1	17	43·38	57·19	86·17	315·15	305·17	1·0078	D.	122·48	86·18	„
1879, Aug. 13·92	4	22	42·58	28·72	49·36	344·39	320·67	0·9700	R.	150·10	129·60	„
1873, July 26·48	1	8	39·84	57·63	85·73	311·03	303·45	1·0106	D.	122·17	85·74	„
1876, Aug. 13·40	1	8	37·77	49·43	83·69	343·73	320·87	0·9730	R.	129·18	96·18	Vásárhely.
1875, July 28·45	3	7	37·58	74·28	63·20	306·06	304·92	1·0152	D.	105·70	63·20	O-Gyalla.
1872, Aug. 10·98	7	26	35·08	66·84	71·85	307·27	318·53	1·0035	D.	112·43	71·94	„
1874, 9·95	1	14	31·96	35·64	62·95	354·30	317·10	0·9102	R.	141·48	115·54	„
1877, 13·44	1	12	30·17	42·60	76·42	2·27	320·63	0·8845	R.	133·39	102·68	„
1879, July 26·96	2	8	29·49	42·67	71·31	315·56	303·52	1·0138	R.	137·14	108·63	„
1872, Aug. 12·42	11	9	28·79	38·68	70·78	7·02	319·88	0·8510	R.	136·49	107·56	„
1874, 9·95	8	28	26·68	47·06	82·96	86·16	317·10	0·9130	R.	129·50	96·68	„
1876, 13·40	3	8	25·63	52·36	87·24	283·77	320·87	0·9102	D.	123·33	87·38	Vásárhely.

Time.	Central No.	No. of Meteors.	l	b	i	π	Ω	q	Motion.	η	$\eta-\theta$	Station.
1876, Aug. 10·92	1	9	24·30	35·97	68·48	14·93	318·47	0·7865	R.	137·30	108·86	Vásárhely.
1872, 10·98	8	12	20·15	53·87	83·69	279·79	318·53	0·9018	D.	121·05	84·05	O-Gyalla.
1875, 11·95	1	22	20·13	8·92	20·93	54·90	318·80	0·4527	R.	149·52	128·64	Szathmár.
1872, 9·97	3	16	19·99	53·27	84·86	278·92	317·58	0·9024	D.	121·81	85·15	O-Gyalla.
1872, Nov. 28·90	1	10	18·50	66·14	27·23	55·08	66·98	0·9758	D.	69·70	27·82	„
1872, Aug. 12·42	13	9	18·45	43·16	84·65	18·66	319·88	0·7686	R.	128·18	94·66	„
1875, July 27·48	1	12	18·44	57·36	84·30	284·15	303·95	0·9850	D.	121·25	84·39	Selmec.
1872, Oct. 24·32	1	13	17·47	64·87	36·42	1·04	31·18	0·9264	D.	83·85	39·01	O-Gyalla.
1872, Aug. 12·42	12	6	16·87	64·38	69·50	231·62	319·88	0·9540	D.	111·10	70·14	„
1879, 12·46	5	13	11·87	39·73	85·92	32·28	319·32	0·6550	R.	127·27	93·28	Fehérvár.
1876, 10·92	2	8	7·34	−0·66	2·66	359·35	138·47	0·1236	R.	138·29	110·42	Vásárhely.
1875, 9·99	3	20	7·08	59·87	70·87	277·76	316 88	0·9000	D.	112·48	72 01	O-Gyalla.
1876, 10·92	6	16	6·19	41·74	86·37	243·99	318·47	0·6423	D.	123·14	87·11	Vásárhely.
1872, 7·42	2	7	5·98	50·07	80·95	259·48	315·08	0·7930	D.	119·63	81·00	O-Gyalla.
1872, 12·01	10	22	1·46	48·55	74·22	254·42	319·50	0·7198	D.	115·93	76·74	„

The radiant 2, 1878, July 29·44, had such an unfavourable situation that the orbit could not be strictly calculated, it has therefore been excluded.

In the comparison of meteor orbits with those of known comets, I found but two which accorded.

	1872, Aug. 7·42 | 1875, Aug. 9·99	Comet 1854, IV. Lesser.	1874, April 20·63 Central No. 2.	Comet 1861, I. Oppolzer.
l	324·30	320·7	267·11	270·6
b	54·02	55·0	56·54	57·0
i	43·77	40·90	78·58	79·75
π	263·21	94·40	172·14	243·37
Ω	315·98	324·47	210·28	209·92
q	0·8117	0·7987	0·8980	0 9207
	D	D	D	D

The cause of it may be partly that all radiant points have northern latitude—that is, that all meteor-streams have been observed in the descending node.

Finally, I must pay my best thanks to Dr. de Konkoly and Dr. Weiss, who were so kind as to aid me with assistance and advice.

Vienna:
1882, *February.*

Observations of the Companion of Sirius, made at the U.S. Naval Observatory, Washington. By Professor Asaph Hall.

Date.	Sid. Time.	p	s	Weight.	Obs.
	h	°	″		
1882, Feb. 27	8·9	42·7	9·93	2	F.
Mar. 2	8·0	42·2	9·88	2	F.
4	6·7	41·4	9·91	3	F.
11	7·0	42·6	9·88	3	F.
13	6·8	42·3	10·12	2	F.
14	7·1	42·3	10·01	3	F.
22	7·6	41·9	clouds	2	H
23	6·5	42·1	9·53	3	H.

Date.	Sid. Time.	p	s	Weight.	Obs.
	h	°	″		
1882, Mar. 24	7·2	43·9	9·79	2	H.
25	6·8	42·2	9·59	3	H.
28	7·2	42·7	9·70	2	H.
29	7·1	41·9	9·61	3	H.
31	7·5	42·7	9·79	2	H.

Results.

	p	s	Observers.
	°	″	
1882·183	42·25	9·955	E. Frisby.
1882·235	42·49	9·668	A. Hall.